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A SCIENTIFIC QUARTERLY DEALING WITH EXTRASENSORY PERCEPTION, THE PSYCHOKINETIC EFFECT, AND RELATED TOPICS

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A FURTHER REPORT ON AN INVESTIGATION OF ESP IN SCHOOL CHILDREN

By J. G. Van Busschbach

ABSTRACT: In the September, 1953, number of this Journal Mr. J. G. Van Busschbach reported results (Series I) from a large number of ESP tests with Dutch primary school children as subjects. The children were successful to a statistically significant degree in identifying ESP target cards which were being looked at by a sender. Two more series based on the same general procedure are reported here. In Series II, the subjects were secondary school pupils and the sender, as in Series I, was a teacher. The results of this series were at the chance level. In Series III, both primary and secondary school subjects were used and the role of sender was taken sometimes by a teacher, sometimes by a stranger, and sometimes by a pupil. The overall results of Series III were significant because of the high positive scoring of the primary school pupils. They had their best scoring with the teacher as sender. The secondary school pupils scored close to the chance level with all senders. The author suggests that the successful combination of teacher-sender and primary school subjects may be due to an especially favorable psychological relationship.

All three series combined (a total of 132,780 trials) gave a statistically significant deviation. The primary school subjects scored significantly higher than the secondary school subjects, and the teacher, as agent, was significantly higher than the other agents.—Ed.

THE ESP TESTS described here were carried out during the years 1950-1954 in the schools of Holland. The subjects were primary and secondary school pupils, and the tests were conducted by the author, who is a government-inspector of elementary and secondary schools in Amsterdam. When the research was started, the main objective was to compare the ability of children in different school grades and age groups. The scoring for boys and girls was also compared, and at a later stage of the investigation the part of agent or "sender" was varied to see whether one type of agent was associated with higher scoring than another.

There have been, to date, three series of tests, all reported in the *Tijdschrift voor Parapsychologie* (**1**, **2**, **3**) and an account of the first series appeared also in the *Journal of Parapsychology* (**4**). A brief review of that series will serve as a background for reporting the other two, since the same general procedure was followed throughout the whole research.

Series I

The tests were presented to the subjects as a game, with rewards of candy for the highest scores. The whole class was tested at one time, and their task was to try to name a series of target symbols which their teacher, acting as agent, tried to "send" to them. The targets were chosen from three different types of symbols selected with the idea that they might appeal to the subjects more than the usual ESP cards. They were arithmetical symbols (+ 0 − : x); colors (red, yellow, green, blue, orange); and words: *stoel* (chair), *bord* (blackboard), *raam* (window), *deur* (door), and *krijt* (chalk).

The teacher sat inside a cardboard cubicle at the rear of the room, shielded from the view of the pupils. He and the pupils were turned back to back. The author served as experimenter and observer. At the start of the tests, the teacher was given a set of the five arithmetical symbols painted on cards which he placed face down on a table in front of him. He distributed them in a haphazard way among place positions numbered from 1 to 5. When all was in readiness, the experimenter gave him a list of 10 random digits numbering from 1 to 5 which indicated the order in which he was to choose the targets before him. The experimenter did not see the order of the target cards. The list of digits was prepared by an assistant and was made up of the last digits of telephone numbers taken from randomly selected pages in a telephone book.[1]

When the experimenter gave a verbal signal, the teacher selected the proper target, turned it over, looked at it for a brief time, and turned it face down again. Each pupil wrote down on his record sheet what he thought the symbol was. Then, at another

[1] See the editorial note regarding the statistical effects of the method of target selection, Vol. 17 (1953) of the *Journal of Parapsychology*, p. 211. In Series II and III the selection of targets was based on a table of random numbers in the Statistical Vademecum by H. Ferro.

verbal signal from the experimenter, the teacher selected the next target; and so on for 10 trials.

The five arithmetical targets were then replaced by five color targets, the teacher was given another set of 10 random numbers, and the procedure was repeated. Finally the word targets were used. Thus each pupil carried out a total of 30 trials at each session. The three kinds of targets were presented in the same order throughout the entire research. The data will not be given, however, because there were no significant differences among the three kinds of targets in any of the series.

The subjects in Series I were children in the primary schools of Amsterdam. They were mostly between 10 and 12 years of age and the classes ranged in size from 20 to 57 pupils. Altogether there were 21 classes with 673 pupils and 21 different teacher-agents, and they made a total of 20,190 trials, with 4,212 hits, which is 174 hits more than the number expected by chance. The CR for this deviation[3] is 2.79 ($P = .005$).

When the data were separated later in the investigation according to the sex of the subject, the results showed that the boys contributed the larger part of the deviation; their scoring was independently significant (11,460 trials; dev. $= +107$; CR $= 2.50$; $P = .012$), but there was no significant difference between their results and those of the girls.

Series II

The second part of the research was carried out in 1952. The subjects were pupils in several branches of the secondary schools and colleges in Amsterdam. In general, their ages varied from 12 to 20 years. There were 21 who were more than 20 years old.

The testing procedure was the same as the one used in the first part of the investigation. Altogether, there were 31 different classes,

[3] The CR's for Series I were computed in the Parapsychology Laboratory by the Greville method for the evaluation of data involving multiple calls on the same targets. The CR's obtained by the Greville method for Series I were about 10 percent less than those obtained when the data were evaluated on the binomial hypothesis, which is appropriate when each call is made for its own randomly selected target. The data of Series II and III have not been available for evaluation by the Greville method, but the relatively slight correction introduced in Series I offers reassurance that the results of Series II and III would not be essentially different by this method, especially since the significance rests upon the same class of subjects as those of Series I.

669 subjects, and 31 different teacher-agents who carried out 20,070 trials. The total deviation (+4) was at the chance level.

When the data for boys and girls were separated, there were no significant results.

Series III

In the third section of the research, both primary and secondary school students were tested. This investigation was carried out in 1954 in the schools of Utrecht.[3] The three-fold aim of this part of the investigation was:

1. To obtain data to compare with the material obtained in the first and second investigations.

2. To discover whether there was a statistically significant difference in the results of tests carried out between a teacher, as sender, and his pupils, and tests carried out between a stranger and the same pupils.

3. To investigate whether there is evidence of ESP when a pupil acts as sender and the other members of his class are subjects.

In this series, the procedure was the same as that of the first and second series except that the role of agent was varied among three types of persons, and the author no longer served as observer. The large amount of travel involved in carrying out the project made it necessary for him to turn the experimenting over to some assistants. A colleague carried out the first five sessions in the author's place. After that, two psychologists, trained as assistants, carried out the remaining 88 sessions, sharing the roles of experimenter, observer, and agent.

All the tests were carried out with the same type of material, in the same succession, on identical forms, in the presence of the same persons, on the same hour of the day as far as possible, and always with each class in its own classroom. Altogether there were 1,175 subjects: 907 from 26 primary school classes and 268 from 9 secondary school classes.

Part of the tests were carried out with 34 different teachers serving as agents. In the primary school this was always the

[3] The Parapsychology Institute of the University of Utrecht, under the direction of Prof. W. H. C. Tenhaeff, collaborated on this section of the research by lending the services of two assistants. The statistical calculations in Series III, like those in Series II, were carried out by the Statistical Section of the National Research Council on behalf of the Foundation for the Study of Psychical Physics, and the results were independently rechecked.

teacher who had full charge of the class all the time. In the secondary schools, it was one of the various teachers who met the pupils for only one class during the day. Another part of the tests in this series were carried out with an agent who was a stranger to the subjects. One of the assistants performed this role. In a third group of tests, one of the pupils from the class was agent—whichever pupil had had the highest score on the preceding part of the tests. The trials with the teacher as agent were always done first; then those with the stranger; and last, those with the pupil.

The primary school subjects gave a significant positive deviation (CR = 3.18; P = .002), while the secondary school subjects gave slightly negative results. The CR of the difference between the two groups is 2.37 (P = .018) which is also significant. Altogether in Series III there were 92,520 trials, with 18,784 hits, and a deviation of +280, which has a marginally significant CR of 2.30 (P = .021).

Table 1 gives the results for the three different types of agent,

TABLE 1

RESULTS OF SERIES III

Subjects	Number of Trials	Hits	Deviation	CR	P
RESULTS WITH TEACHER AS AGENT					
Primary School	26,880	5,555	+179	2.73	.006
Secondary School	8,040	1,608	0	insig.	
RESULTS WITH STRANGER AS AGENT					
Primary School	21,570	4,426	+112	insig.	
Secondary School	7,620	1,490	− 34	insig.	
RESULTS WITH PUPIL AS AGENT					
Primary School	21,030	4,250	+ 44	insig.	
Secondary School	7,380	1,455	− 21	insig.	
RESULTS IRRESPECTIVE OF AGENT					
Total Primary School	69,480	14,231	+335	3.18	.002
Total Secondary School	23,040	4,553	− 55	insig.	
GRAND TOTAL	92,520	18,784	+280	insig.	

for both primary and secondary school subjects, and for the total of all. The primary school children, when the agent was their teacher, were the outstanding subjects. Their results were statistically significant (26,880 trials; deviation = +179; CR = 2.73; P = .006).

When the scores were analyzed according to the sex of the subjects, the results showed that in this series the girls' scoring was independently significant (43,700 trials; dev. = +236; CR = 2.82; P = .005). The difference between boys and girls, however, was not at a statistically significant level.

Combined Results for Series I, II, and III

Since the three series were carried out under the same conditions, it is appropriate to combine the results. Table 2 shows the overall scores for the entire experiment: 132,780 trials, with a deviation of +458, and a statistically significant CR of 3.14 (P = .002). The primary school subjects were responsible for the entire deviation. Their scoring was highly significant (CR = 4.25; P = .00002) in the positive direction while the secondary school children were slightly negative. The difference between the two groups was significant (CR = 2.93; P = .003).

TABLE 2

Series I, II, and III Pooled

All Agents Combined					Teacher as Agent				
A. Primary School					A. Primary School				
	No. of Trials	Dev.	CR	P		No. of Trials	Dev.	CR	P
Series I....	20,190	+174	2.79	.005	Series I....	20,190	+174	2.79	.005
Series III...	69,480	+335	3.18	.002	Series III...	26,880	+179	2.73	.006
Total....	89,670	+509	4.25	.00002	Total....	47,070	+353	4.07	.00005
B. Secondary School					B. Secondary School				
Series II....	20,070	+ 4	insig.		Series II....	20,070	+ 4	insig.	
Series III...	23,040	− 55	insig.		Series III...	8,040	0	insig.	
Total....	43,110	− 51	insig.		Total....	28,110	+ 4	insig.	
Grand Total*..	132,780	+458	3.14	.002	Grand Total†..	75,180	+357	3.25	.001

*For 2517 subjects (1316 boys and 1201 girls) from 87 classes.
†For 86 teacher-agents.

The separation of the total results on the basis of the subjects' sex showed that only the girls' rate of scoring reached a level of statistical significance (63,020 trials; dev. = +282; CR = 2.80; P = .005). The difference between the girls' and boys' scoring, however, was not extrachance.

The comparison of the results on the basis of the three different types of agent showed that with the teacher as agent the CR was 3.25 (P = .001). With the stranger and the pupil as agent there were no significant results. The entire credit for the deviation with the teacher as agent goes to the primary school subjects whose 47,070 trials gave a deviation of +353, with a CR of 4.07 (P = .00005).

Discussion

Presumably the difference between the results with the secondary school pupils and primary school pupils has to be traced to a complex of causal factors. Only a few possible explanations can be pointed out here.

To begin with, there is a difference between the intensity of the contact of the teacher in the Dutch primary school and the teacher of a secondary school. The former occupies himself during the whole school day with one constant group of children; the latter, every other hour, teaches another class, often at another school, and only in one or two branches of learning. He cannot have such an intensive contact with his pupils as the primary school teacher does.

In the second place, the influence of the intellectual instruction in the secondary school does not stimulate the frank answering of questions according to intuitive impressions. The Western system of logical thinking is characterized by the drawing of a conclusion based on premises. The secondary school stimulates that type of thinking and checks the spontaneous giving-way to intuition. The primary school does not follow that way of instruction so intensively and its pupils are less occupied with logical conclusions.

Another possible explanation of the difference between the results of the primary and secondary school children may be the state of the psychical development of the young people of the ages from 14 to 20 years. For a developed mind it is very difficult to draw a conclusion, to state something, without its involving a certain

amount of judgment. That judgment may be good or bad, but it is always there even in the most unconsidered conclusion or statement. The spontaneous following of an intuitive, a-logical impression demands an intensive psychical discipline. On the other hand, a young child, generally speaking, is a more spontaneously reacting creature than a grown-up and he is therefore more capable of following his intuition without psychical exertion.

At first thought, the failure to get significant results in the tests with pupil-agents might seem surprising. However, while it may be true that children understand each other better than grown-ups, yet there is in the relation between educator and child an element of readiness for psychical transfer which is pedagogical in character and which forms the core of the pedagogical act. The so-called readiness does not exist between children themselves nor between a grown-up stranger and a child.

It was not the author's intention to draw final conclusions from these results. The knowledge about both psychological and parapsychological processes is not yet far enough advanced to do this. It was intended only to point out the problem at hand. The investigation produced some interesting results relating to the ESP ability of young people in the test situation, especially with regard to the influence which a familiar and respected agent or leader may unconsciously exert upon that function. Since the results have been produced by such a great number of unselected subjects and teacher-agents, the individual factor has been eliminated to a large degree and we can say that a general phenomenon has been approached.

The results obtained may form a unit; nevertheless they urgently call for incorporation into a larger context with regard to the target material used, the age of the subjects, and the choice of agents, for the experiments here have tested only a very small field of psychical activity. For example, the targets used were only of the cognitive type; those with emotional factors were purposely avoided.

Further experiments will have to deal with other aspects of the problem. The children tested in these experiments were 11 years of age and older. It is important to investigate the reactions of younger children; for example, those from four to six years. Preparations are being made now to test that aspect of the question as

well as to investigate the amount of ESP contact between children and their parents.

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PRECOGNITION OVER TIME INTERVALS OF ONE TO THIRTY-THREE DAYS

By KARLIS OSIS[1]

ABSTRACT: The experiment described here was carried out primarily to provide yet another test of the occurrence of precognition under advanced methods designed to eliminate all other reasonable explanations. The experiment also bears on the question of the extent to which ESP is affected by the length of the time interval which separates the subject and the future target.

There was a single subject in the experiment, and she carried on the tests by correspondence from New York City. Her task was to try to identify ESP target symbols which would be selected and written beside the calls on her record sheet at some unspecified future time by the experimenter in Durham, N. C. Since the subject did not want to know her scores and since there were other circumstances which made it inconvenient for the experimenter to check all the results as soon as they were received, the selection of some of the targets was delayed. The time intervals consequently fell into two groups: from 1-7 days and from 30-33 days. The 239.92 runs gave negative deviations on the direct target that were significant for both the short and long time spans.

Since the use of different time spans arose incidentally, there were differences in experimental conditions that make it impossible to draw any final conclusion from the comparison of the two time intervals. However, the results suggest that precognition may occur over intervals of one week and one month to approximately the same degree.—Ed.

INTRODUCTION

IS ESP AFFECTED by the time interval which separates the subject from the future target? This question is one with which experimenters have been concerned almost since the first efforts to make a laboratory test of precognition. Several studies have involved a comparison of results over different time spans, the longest reported being an interval of ten days.

Hutchinson (3) compared one-day and ten-day precognition. In one series in which the subjects were told their scores as soon as the results were checked, she obtained a significant positive score on the one-day tests and a negative, but insignificant, score on the 10-day tests. The other series, in which knowledge of the scores was withheld from the subjects until the end of the experiment, gave chance results on both intervals.

[1] The author wishes to express his gratitude to the Parapsychology Foundation of New York City whose financial assistance partially supported this research, and to Mrs. Lucie Loughborough whose willing cooperation as the subject in the experiment made the research possible.

Rhine (**5**) compared the precognition scores over one to three days with those over a longer span of 10 days. One of the tests of significance applied was that which measured the consistency of variation of hits within the run and within the segment. The CR of the convariation of salience ratios for the shorter period was 1.37; for the 10-day period, 2.26. Thus the longer interval contributed more toward the overall significance of the results, but a strict comparison of the effect of the time interval was not possible because the tests with the shorter interval were all done before any of those with the 10-day interval.

On the basis of this suggestive result, Rhine and Humphrey (**2**) planned a further precognition experiment to make a closer comparison of time intervals. The runs to be checked after three days and after 10 days were selected on a random basis after the calls were made. The experimenters planned to use the covariation of salience ratios as the primary test of significance; and by this test they got a CR of 1.49 ($P = .14$) for the shorter interval and a CR of 2.89 ($P = .004$) for the 10-day period. Again, therefore, the longer interval did not impose any greater barrier to precognition.

The present experiment was not designed as a test of differences in time intervals, but it does provide results from a shorter time span (1-7 days) and from a longer one (30-33 days). The conditions, as will be described later, were not the same for the long and short intervals, so the results do not afford a reliable comparison of time spans. But at the present stage of the precognition research a comparison is not so important as the more general question of whether *any* evidence of precognition may be obtained over the time spans used. The experiment described in this paper bears first upon the general question of the occurrence of precognition, and secondly upon the question of the time interval that can be bridged.

A general consideration of the results of earlier precognition experiments, both published and unpublished, left the author with the impression that precognition tests with individual subjects might offer more advantages than those with groups of subjects. It also seemed possible that a subject who had reported spontaneous precognitive experiences and who was consequently strongly motivated to take part in a test might possibly do better than the average subject. Furthermore, the secondary or distorted-scoring effects (dis-

placement, for example) which appear to be more characteristic of precognition than of other modes of ESP, are likely to vary from one subject to another. They might show up in the work of an individual subject, whereas they might cancel each other out or be obscured in the work of a group of subjects.

The experiment with one subject, described in this report, was planned with the above assumptions in mind, but it was not intended to provide a test of them. The primary hypothesis to be tested was this: would statistically significant results occur in this experiment with this selected subject when conditions appeared adequate to exclude every other reasonable non-chance factor than precognition?

Procedure

The subject of this test was Mrs. L. M. Loughborough, formerly an opera singer, of New York City. In her correspondence with the Parapsychology Laboratory Mrs. Loughborough reported several spontaneous experiences involving precognition. Three years ago she became interested in parapsychology, joined the American Society for Psychical Research, and read widely on the subject. She expressed an unusually strong desire to participate in ESP experiments. In fact, her first letter to the Laboratory communicated this desire, and in the course of further correspondence she gave assurance of patient determination to go on with the tests even if they should prove to give only chance results.

On the basis of this correspondence an experiment was designed which was carried out in the period from August, 1954, to March, 1955. The entire test was conducted by mail. The experimental plan was as follows: There were to be four series, each having 12 short sessions of five runs each. The evaluation of the total deviation and an analysis of variance were planned, though these were not discussed with the subject and she was given only the necessary instructions regarding the number of runs to do per session. However, the subject did not follow the instructions to the letter, probably because the reasons were not fully explained. Therefore, even though we were able to carry out the work to the total length planned, the structure of the experiment in uniform units of runs, sessions, and subseries was not maintained. The sessions varied in length from 10 to 49 runs. The original limit set for the length

of each subseries had to be disregarded, and it became impracticable to apply the statistical method of analysis of variance to the results.

The standard printed record sheets with 10 runs were used. The subject was instructed to guess the ESP card symbols which would be inscribed beside her calls at some indefinite future time. No actual cards were involved in the experiment; the targets were merely the randomized symbols as they were determined later and written into the "card" columns of the record sheet.

On one occasion the subject was out of record sheets, and she made five of her own consisting of seven to eight runs of 26 to 31 trials each.

The subject was asked to do the test when she felt she could do it well. In the beginning she did a considerable number of runs at a time (up to 49) during the evening at home. Later, she did one and sometimes two sheets in her office before starting work in the mornings. Sometimes she complained that the tests caused headaches.

The record sheets were mailed to the experimenter at the Parapsychology Laboratory in Durham, N. C. After they arrived he made, at his convenience, an entry into a random number table, using a procedure designed to exclude subjective influence. The digits, taken in order from this point, were translated into the target orders and were inscribed on the sheets. These target orders were determined by a standardized method which was first described in detail by G. L. Mangan (**4**). Essentially, the method consists of throwing dice to obtain four three-digit numbers, and of manipulating these numbers according to certain rules by means of an electric calculating machine to such an extent that no human mind, presumably, could arrive at the end result without having used some kind of mechanical aid (e.g., pen and paper). The purpose of the calculation is to impose a barrier against the possible use of clairvoyance and PK to influence the place of entering the random number table to find targets that would match the subject's calls. By complicating the process beyond the possibility of using inference, any mode of psi on the part of the experimenter would, it appears, be ruled out as a factor in any results obtained.

The end result of the procedure with the dice plus the calculating machine determines an entry into a random number book (**9**).

The numbers following the entry were used without any break for all the sheets which had been received and which remained unchecked at the time of randomization. The chronological order of the calls was followed. The number of runs randomized on the various occasions were: 81, 39.68, 29, 23.28, 17, and five groups of 10 runs. On the occasion when the 81 runs were randomized, the entry into the table of numbers was determined in the experimenter's presence by Miss Catherine Thompson of the secretarial staff, who used the method indicated above.

The instructions to the subject were that she make her calls for targets that would be written on the record sheet at some time in the future. No time interval was specified. At the beginning of the experiment the subject requested that she not be informed regarding her scoring. In view of these circumstances the experimenter felt no need to check the results immediately after receiving the subject's call record, but did so at his convenience. At the beginning of the series the experimenter usually delayed the selection of targets and checking for about a month, but subsequently he was able to check the results soon after the calls were received, that is, after an interval of one to seven days. Also, in the later part of the experiment the subject was told her results in the hope that her motivation would be more favorable for making significant scores.

Table 1
GENERAL RESULTS

Time Span in Days	Runs	Hits	Dev.	Aver. Score	CR	P
1-7	171.24	791	− 65.2	4.62	2.49	0.13
30-33	68.68	304	− 39.4	4.43	2.38	0.17
Totals	239.92	1095	−104.6	4.56	3.38	.0007

RESULTS

The general results are presented in Table 1. There is a total deviation of −104.6, which is highly significant ($P = .0007$). The scores on short (1-7 days) precognition periods and on one-month periods (30-33 days) are both statistically significant ($P = .013$ and $P = .017$). The average score for the one-month period is

slightly more negative, but the difference between the two is well within the range that may reasonably be attributed to chance variation.

The number of runs checked on each occasion of entering the random number table apparently had no effect on the score. The average for the 50 runs which were checked in five groups of ten runs each was 4.66, while the 189.92 runs checked in larger groups averaged 4.51. The difference is without statistical significance.

The negative deviation appeared consistently throughout the series, as shown by the fact that of a total of 27 record sheets used, 21 gave negative deviations, four gave positive deviations, and two gave no deviation from mean chance expectation.

Several other analyses were carried out. In previous work (**1**, **4**), precognition has sometimes been associated with +1 displacement. No evidence of forward displacement was found in this experiment. The horizontal distribution of hits on the page was also tested and no decline was found.

The vertical distribution of hits in terms of the five segments

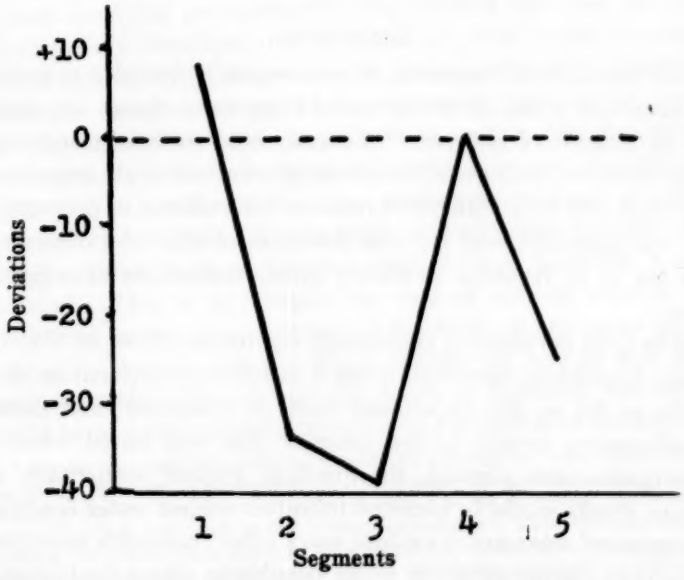

FIG. 1. Graph showing distribution of hits in the five segments of the run.

of the run was evaluated on those 200 completed runs where record sheets with columns of 25 were used. The broken run and the runs on the self-made record sheets with varying lengths of columns could not conveniently be evaluated in terms of segments of five trials, and they were excluded from this analysis. The remaining runs were pooled. The distribution of hits for the first through the fifth segments, respectively, was: 208, 167, 161, 200, 175. A chi-square test shows that these figures depart to a marginally significant degree from the expected rectangular distribution ($\chi^2 = 11.51$; 4 d.f.; $P = .02$). This statistical test is, of course, independent of the evaluation of the total negative deviation. The vertical distribution is shown graphically in Fig. 1. It closely approximates a U-curve, which has often been reported for ESP data though usually with positive scoring. There is a falling-off in the fifth segment which has been called "the broken wing effect." It is interesting to note that the main deviation was largely contributed by segments 2 and 3, less from segment 5, but none at all from segments 1 and 4.

Discussion

As stated at the beginning, it was originally intended to evaluate the results in terms of the deviation from mean chance expectation and an analysis of variance. The analysis of variance could not be made, however, because of the mixed-up structure of the experiment. Therefore, the only anticipated measure for evidence of precognition that was applicable was the total deviation of hits. All other measures are to be regarded as merely giving indications of suggestive value.

The total deviation is statistically significant ($P = .0007$). The chance hypothesis, therefore, is not a satisfactory explanation of the results so far as this experiment alone is concerned; and there is an affirmative answer to the question that was raised when the investigation was planned, the question whether statistically significant results would be obtained from this subject under conditions that appeared adequate to exclude every other reasonable non-chance factor than precognition. It is not possible to give a final interpretation of this result in terms of its bearing upon the cumulative evi-

dence of the precognition hypothesis,[2] but some pertinent questions can be raised and answered in approaching such an interpretation.

(1) Can recording errors be excluded as a counterhypothesis? The data of this experiment were submitted to an elaborate rechecking to insure against such errors. The calls were written in ink by the subject. The recheckers found four of them rather difficult to read, and a person who had not seen the targets was asked to identify them. In one case he disagreed with the experimenter's interpretation, but his judgment was taken as final. All the operations of the electric calculator, which was used for determining the entry into the random number tables, were independently rechecked. No mistakes were found. The target order was independently rechecked against the random numbers used. The scoring also was independently rechecked. A few mistakes in both targets and scoring were found which had the net effect of reducing the deviation by one point.

(2) Were the targets determined in such a way that the results could not be interpreted in terms of non-precognitive psi or reasoning ability on the part of the experimenter or the subject without involving precognition? The method used was the standard procedure developed over a period of years in this laboratory. As Mangan recently pointed out (**4**), it is a sufficiently reliable procedure for the present stage of experimenting, but probably not the last step of development in methods of testing for precognition. One counterhypothesis is the selection of a favorable entering point in the random number table. The calculation interposed is intended to guard against this. If it is really not adequate, however, there is still another basis of judging whether such favorable entering occurred. This is to compare the rate of scoring when a small number of targets were selected on the basis of one entry into the table with the rate when a larger number of targets was drawn from the table. The shorter the series of numbers, the easier it would be to make a favorable choice of a sequence of random num-

[2] During the past few years, a number of precognition tests have been conducted by different experimenters in the Parapsychology Laboratory with the general procedure for target selection, described in the preceding report by Mangan (**4**), which was used in the present experiment. Some of these tests also involved the use of time intervals of a month or even longer. A survey of all these results from the point of view of the cumulative evidence for precognition as well as the effect of the time interval is being prepared, and this report should be ready for early publication.

bers for the targets. In this experiment, however, the results were just the reverse of what one would predict on this hypothesis: the longer target selections gave a slightly larger average negative deviation (−.34) than the shorter selections (−.49).

It is interesting to point out that this precognition experiment has yielded a significant total deviation on the direct targets, the first such case, apparently, among precognition experiments using advanced methods. The main results have hitherto been found in an even less direct way through such secondary effects as displacement. This fact suggests that extrasensory perception does not come through directly, but is distorted, either by subjective or objective factors, in a laboratory test. In the present case, distortion can be seen in the negative sign of the deviation, the psi missing.

The easiest explanation of some types of psi missing discovered long ago is of course unconscious negativism or motivational conflicts (**6**). In the present case, however, the negative deviation is achieved in only three segments of the run. The first segment gave positive results. Rhine suggests that such variations are less easily explained as volitionally produced results than as some sort of distortion in cognitive processes themselves (**7**). Whether the distortions in precognition experiments are caused entirely by psychological factors, or perhaps by the time variant involved is a problem that requires experimental study.

In the present investigations the comparison of time spans was not planned as a part of the original experimental design; the use of different intervals arose incidentally in the course of the series. There were other differences in conditions, therefore, that might have affected the scoring under the long and short time spans: the 30-33 day tests were largely completed before the 1-7 day tests began; the subject was told her scores during the shorter interval period; and the length of calling sessions varied widely. Therefore no valid comparison of the two time spans can be made, but the fact that the 30-33 day tests contributed in an equal degree with the 1-7 day tests to the overall significance of the results offers a strong suggestion that precognition occurs in the test situation over at least a one-month interval. The results therefore offer encouragement for further research specifically planned to bear upon the problem of long-time precognition.

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A SECOND REPORT ON PSI EXPERIMENTS WITH AUSTRALIAN ABORIGINES

By RONALD ROSE[1]

ABSTRACT: The research reported here is a continuation of a previous series of tests carried out by the author among a group of Australian aborigines. The same procedure was used in both experiments: the subject, observed by an experimental assistant, tried to identify ESP cards which were being looked at by the experimenter in another room. The 139 runs carried out in the new series gave highly significant total scores. The percentage of significantly high-scoring subjects in this series was greater than in the first, in which one high-scoring subject contributed most of the extrachance results. This same subject was the highest scorer in the current experiment also.—Ed.

THE REPORT PRESENTED here is part of an investigation which my wife and I have been making of a group of aborigines at Woodenbong in northern New South Wales.[2] Our investigation was devoted partly to collecting anecdotal material and information about aboriginal psi practices and beliefs (1, 3) and partly to carrying out ESP and PK tests. Some of the experimental work has already been published in this Journal (2); the present report is a follow-up study.

ESP TESTS

The experimental setting in this later series of tests was much the same as that which applied during the preceding series. The subjects were semi-civilized natives, mostly half-caste or less, who had been absorbed into the white community. Because of deaths, illnesses, etc., less than half of the subjects from the previous series were available. Altogether 12 subjects were tested, all but one of whom had been subjects on previous occasions. One of the outstanding scorers in the first series (Clara Williams) died during the new series.

[1] The author wishes to express his thanks to Mr. and Mrs. J. B. Stratton, the Manager and Matron of Woodenbong Aboriginal Station, for their generous assistance.

[2] This paper is adapted from a report to the Parapsychology Foundation who have partly supported the work, and it is with their kind permission that we are publishing it here. An expanded version of the entire investigation, written in popular style and entitled *Aboriginal Magic*, has been prepared by Mr. and Mrs. Rose for possible publication in book form.—Ed.

The tests were done by the GESP method, with a deck of 25 ESP cards. Usually those subjects who acted as receivers or percipients were in one room of a cottage accompanied by Mrs. Rose (L. R.), who recorded their guesses. R.R. acted as agent or sender, and he was stationed in another room or on the verandah. The cards were shuffled by hand by L. R. between runs, always out of sight of the percipient. Just before turning the top card at the beginning of each run, R.R. called "Right" so that L.R. could tell the percipient to start calling. The fact that R.R. could hear the percipient's responses served as a guide for him to turn over a new card for each trial. At the conclusion of a run, he took the cards to L.R. and called through their correct order, which she recorded against the percipient's guesses. Hits were checked off and the total was recorded. There usually followed a short discussion on the merits of the run, with encouragement for further effort. Usually only three or four runs took place at each experimental session, and if a subject said he did not wish to do any further work on that occasion his wishes were respected. This, it is thought, contributed somewhat to the results obtained.

As previously, the possibility of sensory cues of any sort was thoroughly investigated before the tests began. The few onlookers who were present were cautioned about the need for silence and were never in a position where they could have aided the percipient by signalling. Percipients and onlookers complied with the conditions on all occasions and no attempted deception of any sort was detected.

The author appreciates the fact that the conditions of this field investigation would not meet the stringent requirements of contemporary laboratory testing, but it is believed that they were adequate; that is, that the subjects were effectively isolated from sensory knowledge of the cards, that the hand-shuffling adequately randomized the deck before each run, and that recording was free from error.

General Results

Twelve subjects were tested, and they carried out a total of 139 runs. There were 938 hits, with a positive deviation of 243 (CR = 10.31; $P < 10^{-22}$). The CR of 6.57 in the series previously

reported came largely from the excess hits obtained by one outstanding subject, Lizzie Williams. In the present series, the results were more satisfying in that more than half of the subjects obtained total scores that are independently significant. The results are given in Table 1.

Table 1
GENERAL RESULTS

Subject	Runs	Hits	Dev.	CR	P
Lizzie Williams	32	236	+76	6.72	$< 10^{-9}$
Clara Williams	10	74	+24	3.80	.0001
Digger Marine	10	74	+24	3.80	.0001
Harry Monsell	9	67	+22	3.67	.0002
Rene Robinson	28	178	+38	3.59	.0003
Bert Mercy	11	74	+19	2.87	.004
Nellie Charles	17	108	+23	2.79	.005
Eileen Mitchell	12	74	+14	2.02	insig.
Wallace Monsell	2	14	+ 4	1.41	insig.
Walter Page	4	21	+ 1	0.25	insig.
Owen Morgan	1	5	0	0.00	insig.
Merle Bundi	3	13	− 2	0.58	insig.
Totals	139	938	+243	10.31	$< 10^{-22}$

Results of Highest Scoring Subjects

The results of the five highest scoring subjects have been analyzed in some detail.

Lizzie Williams. As before, the data of this old lady, now 79 years of age and bedridden for the last four years, are most interesting. The chronological distribution of hits in the four quarters of the experiment shows very high scoring in the first portion and a marked decline toward the end of the series. (See Table 2.) The difference between the first and fourth quarters is significant ($CR_d = 2.38$; $P = .02$), and the difference between the first and third quarters is even more so ($CR_d = 3.00$; $P = .003$). The chi

Table 2
CHRONOLOGICAL DISTRIBUTION OF HITS IN THE DATA OF LIZZIE WILLIAMS

	Runs	Hits	Dev.	CR	P
1st Quarter	8	71	+31	5.48	$< 10^{-7}$
2nd Quarter	8	66	+26	4.42	.00001
3rd Quarter	8	47	+ 7	1.23	insig.
4th Quarter	8	52	+12	2.12	insig.

square of the distribution of hits for the four quarters is 6.60 (d.f. = 3), which gives a P value of between .05 and .02.

A comparison of the subject's chronological performance in this series with her performance in the previous series shows an interesting resemblance in the general pattern of decline. This is illustrated in Figure 1.

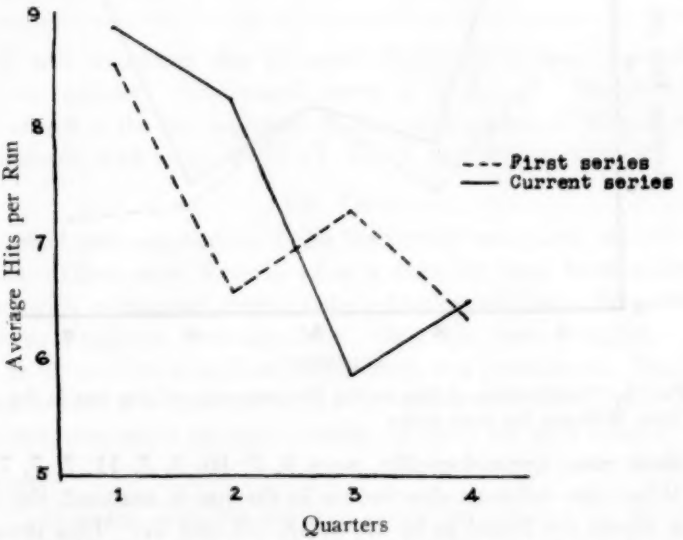

FIG. 1. Chronological distribution of hits in the four quarters of the experiment in the data of Lizzie Williams, for both series.

There is some resemblance also in the subject's distribution of hits in the runs for the two series. When her runs in the current series were pooled and then divided into five-call segments, the deviations for the segments were +27, +9, +11, +7, and +22. This represents a rather striking U-distribution of hits, as shown in Figure 2, which compares the vertical distribution in the two series. There was only slight beginning salience in the first series, but terminal salience both of the beginning and end of the run occurred in a marked degree in the second series.

Clara Williams. The next highest scoring subject was Clara Williams, whose 10 runs resulted in a CR of 3.80 (P = .0001). Unfortunately she died during the current series, and analysis is therefore limited to the few runs she had completed. The scores

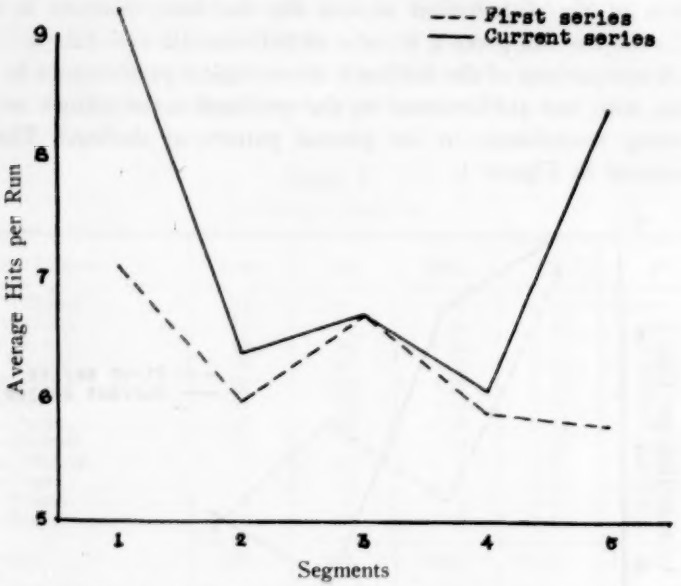

FIG. 2. Distribution of hits in the five segments of the run in the data of Lizzie Williams, for both series.

on these runs, chronologically, were 9, 7, 10, 3, 7, 11, 7, 7, 7, 6.

When this subject's distribution in the run is analyzed, the segment scores are found to be 17, 8, 18, 10, and 21. This unequal performance calling through the deck approaches significance ($\chi^2 = 6.2$; d.f. $= 4$), giving a P value of between .01 and .02.

Digger Marine. This subject's work was also prematurely terminated, for he left the district after completing only 10 runs. These, however, yielded a CR of 3.80 ($P = .0001$). His run scores, chronologically, were 10, 8, 9, 6, 5, 9, 7, 7, 3, 10. His distribution in the run is much more uniform than those of the previous subjects, the score for the five segments being 10, 20, 15, 14, 15, respectively. The variation is not significant.

Harry Monsell. This subject had a high overall score, but no special scoring effects.

Rene Robinson. The scoring trends of this old lady, whose ESP work produced a CR of 3.59 ($P = .0003$) in 28 runs, are most interesting. The chronological distribution of hits is shown in Table 3.

Table 3

CHRONOLOGICAL DISTRIBUTION OF HITS IN THE DATA OF RENE ROBINSON

	Runs	Hits	Dev.	CR	P
1st Quarter	7	57	+22	4.10	.00004
2nd Quarter	7	43	+ 8	1.51	insig.
3rd Quarter	7	34	− 1	0.19	insig.
4th Quarter	7	44	+ 9	1.70	insig.

It will be noticed that the most significant scoring occurred in the first quarter. The overall curve is U-shaped. The distribution of hits in the five segments of the run, however, is comparatively uniform, with totals of 32, 43, 30, 34, and 39, respectively.

PK TESTS

Inlaid dice supplied by Duke University were used for the PK series. These were thrown, 12 at a time, by hand from a shaker lined with corrugated rubber onto a blanket laid on a flat surface, such as a table or verandah floor. Only hits were recorded.

In all sessions a spirit of competition was encouraged. Natives, three or four at a time, threw in competition with each other. Each die face was target an equal number of times for each subject.

Results:

Once again, as in the previous series, no evidence for the existence of PK was obtained. Table 4 gives the general results.

An interesting feature of the subjects' performances is that Nellie Charles, the best scorer in the first series, is the worst performer in

Table 4

GENERAL RESULTS

Subject	Runs	MCE	Score	Dev.	CR	P
Naomi Holton	24	96	106	+10	1.12	insig.
Evelyn Brown	24	96	103	+ 7	0.78	insig.
Tom Kenny	60	240	249	+ 9	0.64	insig.
Harry Monsell	96	384	391	+ 7	0.39	insig.
Owen Morgan	36	144	148	+ 4	0.37	insig.
Wallace Monsell	24	96	98	+ 2	0.22	insig.
Shirley Cavanagh	12	48	39	− 9	1.42	insig.
Eileen Mitchell	24	96	83	−13	1.45	insig.
Nellie Charles	12	48	37	−11	1.90	insig.
Totals	312	1248	1254	+ 6	0.19	insig.

the current series. In neither case does her deviation reach the level of significance.

Discussion

The results provide evidence of reasonably widespread ESP in the group of aborigines tested. Variation of technique, such as the exchange of the functions performed by my wife and me, the use of a matching procedure, and clairvoyance tests should be carried out for whatever worth they may have in throwing further light on the processes involved.

The failure to obtain evidence of PK is disappointing. A number of reasons might be advanced to explain this, the main ones being the relative physical indolence of the aborigines (as compared with ourselves or other native peoples, for example) and their lack of initiative and drive in their everyday affairs. The experimenters believe that during the tests the optimum conditions existed for the demonstration of PK if it were present.

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EDITORIAL

SOME PRESENT IMPASSES IN PARAPSYCHOLOGY[1]

While we who work in parapsychology may properly speak to our more general public with a certain legitimate optimism regarding the general progress that has been made in our field, we ought, I think, among the closer circle of fellow students which this Society represents, to take a graver view of our situation. What should be emphasized, of course, depends upon one's purpose. At times one may properly build up his confidence by casting a reflective eye over the advances made in the past; he may then correct whatever bias this encouraging perspective has given him by turning to face the unsolved problems ahead and considering the slow harvest of the current research. To make a sobering survey of the difficulties confronting us, however, is more an act of realism than of pessimism, and it would be pointless if it were not done with a constructive impulse, a searching for new ways out of the existing impasses and frustrations.

It is not at all difficult for those who are close to the research front to catch this serious mood. More and more as one seasons himself with experience in this branch of research, its formidable difficulties impress themselves upon him. So much is this the case that it is well, perhaps, that the realization of it comes only gradually. In view of this somewhat depressing trend of experiences, which is by no means an individual matter, it seems important to specify some of the more troublesome points that confront the parapsychology workers and to try at least to appraise them.

One of the major checkmates which workers in parapsychology have to face today is the fact that the prevailing culture of general science is more vigorously and persistently opposed to parapsychology than could have been supposed. The physicalistic concept of man and life, and the whole habit of thought that goes with it, have become a faith to the student of science, and even the extraor-

[1] This editorial is based on a paper prepared by J. B. Rhine for presentation to the members of the Society for Psychical Research in London on July 28, 1955.

dinarily high standards of evidence set up in our field are not enough to counteract the blinding influence of this established belief. While individual workers can, of course, survive and carry on in spite of such resistance, this point of view has serious consequences to the field as a whole. It definitely influences those who make decisions on research funds, who control publication and who provide laboratory facilities, and even those who are contemplating research in parapsychology as a career. Such a state of affairs makes it more difficult for a laboratory staff devoted solely to parapsychology to be assembled and to be maintained.

What prospect there is of overcoming this resistance to the results of psi researches on the part of professional scientists is not easy to assess. The possibilities are difficult to rank. For example, today more than ever before there is promise that the practical application of psi may be realized, at least on some sort of marginal basis. As Dr. Schiller pointed out long ago, such application should be expected to expedite general recognition of psi. Those who are especially interested in this still uncertain prospect should watch for a report of research by Dr. Remi Cadoret in a forthcoming issue of the *Journal of Parapsychology.*

The second possibility is of a very different type: An impressive rationale is now developing around the findings of the psi investigations, and this rational framework makes psi much more acceptable to most people. I suspect, however, that this growing rational organization of what is known about psi probably has more value in sustaining conviction already in existence about the reality of psi than in engendering such conviction in areas unreceptive to it. The strongly biased individual will simply not look at this rational picture, and thus it does not affect him, no matter how potentially compelling its effect would be if he were familiar with it.

There is a third factor that could, with a certain turn of affairs, bring parapsychology closer to the center of interest in Western science. That is the degree to which the findings of the psi researches constitute a refutation of materialism. Anything that could effectively dramatize the extent to which this one-sided view of reality limits our knowledge of the nature of personality might quickly bring parapsychology out of its comparative obscurity and put it to work in laboratories around the world, just as the dramatic

threat of atomic power resulted in concentrated research in that field a dozen or so years ago. Is it not possible that a vigorous impulse to re-examine the philosophy of materialism may be generated as the Western world becomes more and more menaced by total destruction, the means to which lies in the hands of men who base their actions on a mechanistic philosophy of life?

Revolutionary turns of thought have in the past depended more upon human events than upon human reasoning. The same will probably hold true in determining which factor will most effectively focus attention on parapsychology—events will decide! In any case, whatever eventual recognition is finally achieved will depend greatly on the extent to which the general lay public is familiar with the results of the psi investigations. The educational activities which link parapsychology with the public, then, are essential to the existence of effective research in parapsychology.

* * *

There are, of course, frustrations in the researches themselves. Some of the blocks to progress in parapsychology may be due to nothing more than to the habit of taking a traditional boundary line too seriously. The fact that we are now more or less stopped in our tracks in research on telepathy may be due to that habit. Person-to-person transfer of thought by extrasensory means is now relatively well established, and parapsychology waits for general psychology to analyze personality to a point where a further refinement of telepathy tests is possible. The evidence of telepathy as it now stands is capable of several interpretations, as Thouless and Wiesner have indicated. But no one seems to have in mind a method by which to distinguish, by means of experimental design, between the alternatives. Meanwhile, the original hypothesis—a hypothesis that is of significance in many areas of thought—that the transfer takes place directly from mind-to-mind, stands waiting until psychology and neurophysiology fill in the gap of knowledge that would make the next step on telepathy research possible.

Yet this situation may be just a further opportunity for parapsychology. If, on its own initiative, the research could establish a case for telepathy on the purely mental level, it would be contributing to psychology and neurology what could very well be their most important fact, instead of waiting for that fact to

come from them. But of course such mind-to-mind explanation of telepathy may not be the correct one; it might be hard to prove even if it were, and it could be well-nigh impossible to disprove.

At all events, there *are* certain leads—actual possibilities to be explored—as to how to probe further into this problem of the nature of telepathy. One lead is suggested by cases of apparent telepathy from man to animal. Consider, for example, the case of a German Shepherd named Lucky, whose master, hundreds of miles away, was in an airplane crash and was left lying unconscious in the wreckage. As later information brought out, Lucky disappeared from the yard about the time of the crash. He crawled through a small opening under the house, which he had never been known to use before, and would neither come out when called nor eat the food presented. He lay unresponsive, seemingly dazed, paying no attention to commands or pleading, during the approximately twenty-four hours in which his master was unconscious. When the man recovered consciousness, however, in the distant hospital the next day, Lucky got up, came out from under the house, and ate and behaved in his normal manner.

There are, of course, several possibilities upon which the speculative mind may seize to explain the coincidence between man and animal in the many cases of this type on record. Variations in the material itself rules out some of them. In not all of the cases is the human companion unconscious as in this one. He may be only partly conscious, as in a struggle with pneumonia. It seems reasonable to suggest that the agency in such cases may not be wholly on the side of the man. The dog's motivation is more likely to be the primary one, during the period of serious illness or unconsciousness of his master. If such be the case, the interpretation would have to be either that there was a mind-to-mind relationship or else that the dog perceived his master's condition by clairvoyance. But even granting the dog perfect powers of clairvoyance, he would have trouble, not having any medical knowledge, in distinguishing between his master in an unconscious state in a hospital bed and the same master asleep in a hotel room. It is doubtful if the dog would even know the physical difference. It is easier to suspect that the dog Lucky was in some effective mental way bound to his human companion and that he was distressed when that physical attach-

ment became strained and almost broken. This is only a suggestion, but it introduces a new angle of approach.

Just next door to this category of animal cases is the rather large number of death-howl cases in which the animal gives way to a pained, mournful howl (or moan or sob, as different reporters describe it) when the death of his master (in many cases far distant) occurs. This would suggest that in such instances some invisible relationship existing on the subjective level must be broken. In many instances determination of the precise time of death would require technical knowledge and a degree of sophistication that the dog could not be expected to have.

Cases of this type suggest the possibility of differentiating in a more delicate way than has so far been possible between telepathy and clairvoyance. The point is that there *seems* to be a communication of feeling rather than a cognition by the animal of the delicate physical change involved in unconsciousness or death. With the clue furnished by such cases as this, the ingenious experimenter may very well think of a test that would be even more discriminating than that suggested by the dog anecdotes. It need hardly be said that a new opening to a more penetrating level of research in telepathy would be an epochal event in parapsychology.

* * *

I turn now to take note of the other currently checkmated situations in parapsychology with which I might be expected to deal on this occasion, but which, for reasons I shall mention, cannot properly be taken up at this time. There is, for example, what must be for many of us in this room the most frustrating impasse in all parapsychology's history—the state to which research on the spirit hypothesis has come. Interest in this hypothesis may properly be credited with having brought about the founding of parapsychology as a branch of study. Yet, during the last twenty-five years, the question as a project for scientific research has rapidly declined rather than developed in status and progress. But since I shall deal with that topic in tomorrow's lecture at the College for Psychic Science, I shall now say only that at the present it represents the most conspicuous failure of the entire branch of inquiry, and it is, in my judgment, in need of a thorough-going

reconsideration as a problem if it is to be successfully revived and investigated.

I shall likewise, although for other reasons, pass over other blind alleys on which workers in parapsychology have wasted many precious man-hours of research time because they have yielded no outcome, pro or con. Let us take, for example, the problem area designated as spiritual or unorthodox psychical healing. This may conceivably and even reasonably be regarded as a border zone of psi operation. At this stage, however, it cannot be claimed either as a part of the legitimate realm of parapsychology nor yet dismissed as definitely not belonging in any way to the domain. As a matter of fact, after many decades of half-heartedly looking into this area, we are scientifically very little the wiser or surer about whether or not psi enters to an appreciable extent into the effects reported.

There is the less important but no less extensive and ambiguous zone of problems associated with automatisms, those unconscious muscular movements that involve the use of the forked twig, the pendulum, or those more modern equivalents, the "dual locators." I pass over these last mentioned areas of what might be called applied parapsychology for the reason that at so exploratory a stage as our science still finds itself in today, these projects should not properly be pushed ahead of the more basic research problems. The latter are already far too great for the limited personnel and facilities available. It would be justifiable to take up the claims of practices such as healing and dowsing, I think, only if they actually gave promise of yielding readily to demonstration, thereby allowing an experimental analysis of the factors involved. That, unfortunately, is definitely what they do not do. When conditions of proper control have been imposed, they have not thus far given better evidence to the parapsychologist than have the more easily handled purer types of psi manifestation. We must always keep in mind this historic fact about these areas of practice involving psi: had they been successful in a practical way to any marked and reliable degree, they would long ago have brought the principles they imply into adequate recognition. That problem, at least, would have been solved ages ago.

* * *

There is, however, one major impasse fairly obvious to all of us who have engaged in parapsychology experiments, and it is a blockage of such magnitude that one might almost say that if it is not solved effectively and soon, the future of parapsychology is not a very bright one. It is the problem of psi control, a problem that is evident from the fact that after all the hundreds of researches that have firmly established psi as a natural phenomenon, as yet no very effective control over it can be exercised. All experiments in psi naturally depend on the recurrence, however irregularly, of the phenomenon investigated, and the more this recurrence can be controlled and regulated or the more voluntary it can be made, the more successful the experiment will be. It is a fairly disturbing fact, however, that not only does the subjects' ability tend to decline, but even experimenters who have been associated with psi phenomena a long time can often no longer succeed as well as they did initially. The net effect with regard to being able to obtain a successful demonstration of psi on demand is that the beginner today is not very far ahead of his counterpart of fifty years ago.

It is true, and it is important, that today we do know better than was known a half-century back what the trouble really is. Knowledge that psi occurs on an unconscious level gives enlightenment to the experimenter, but not emancipation from the limitations of that fact. Some acquaintance with the range of target variety and recognition of the voluntary character of psi and of its apparent independence of physical conditions enable the experimenter today to design better experiments and to control them better than his predecessor of earlier decades. Likewise, our knowledge of a wide variety of the psychological conditions and personality states that affect the level and trend of scoring in psi tests or that produce odd effects in the data make it possible for us now to detect the working of psi in many experimental series that would otherwise be dismissed as due to chance. But there still remains the fact that, among those who are sincerely interested in demonstrating psi experimentally, there are many who have tried in vain, even right from the start. And it is not easy for those who have been more successful to provide them with a simple, adequate explanation for their failure or to offer a more dependable course of procedure.

Now this is an especially serious block to progress in parapsychology because all other and later experimental developments depend first of all upon the successful production of psi effects. General recognition of parapsychology, of course, is retarded because of the fact that many well-meaning experimenters have experienced failure in getting the order of results that others have reported, even though they went through the same overt procedures and thought they used the same conditions. Obviously, progress in the whole area is hobbled by the lack of a reliable art of producing psi effects. And finally, to cap the climax of this frustration, experimenters who have been successful and have gained valuable experience may find themselves unexpectedly inactivated by acquired inability to get results. Thus, as one surveys the trend outlined, he can hardly escape the frustrating outlook that it gives.

Such a situation, lasting as long as it has and extending as widely as it does, is not to be corrected by a simple suggestion. It is fairly obvious that what is going on is something general and fundamental enough to warn us not to expect a quick and easy solution. It is even possible, though unlikely, that there is no solution. On the other hand, we all think of psi as a natural phenomenon and it seems quite obvious, therefore, that some experimenters will succeed where others fail, and that some will succeed under certain conditions and fail under others. These differences, of course, are a part of the causal system of nature and all we need to do, presumably, is to find out what causal operations are going on and adjust our practice intelligently to the expanded knowledge. As it is, there are certain aspects of the test situation over which we do have some control, and we can surely hope to learn to command the natural, lawful factors involved in psi tests better than we do at present. It is in that hopeful possibility that I shall consider with you a few suggestions—some that may also have occurred to others who have been giving thought to this problem.

May I submit first that the most important need is a fundamental shift of emphasis on what a psi test should be considered to be. Such a test is at best an artificial set-up, designed to capture an unconscious and spontaneous phenomenon. While the subject does exercise a certain voluntary effort, we know little of how to tell him to go about it. He has no reliable introspective light on

his success or failure. Nevertheless, some sense of conviction or certainty may be possible in extreme psi cases. In the studies of L. E. Rhine it was shown to be very common in spontaneous cases of psi. If the test subject could somehow be got into a highly motivated state comparable to that in which spontaneous psi is experienced, naturally he might much more easily have a feeling of conviction or certainty. But it is almost impossible to achieve this in a controlled test situation. In fact, if that were an essential requirement, it would almost look as though psi were not a phenomenon subject to successful experimental study.

Yet this picture can be turned around to a better angle. We can get the suggestion from it that we may have been coming at the problem of demonstrating psi in the wrong way, because of the fact, perhaps, that we naturally tend to follow the analogy of sensory perception. Why should we not, from now on, simply say that a test is a psi test *only* when it meets the natural requirements of this elusive spontaneous process? For example, psi in everyday life may ordinarily require a certain force of motivation. A psi *test,* then, should automatically require it too. Thus, by refusing to impose on a subject the sort of test we have no reason to think is a valid one, and on the other hand by expecting psi to occur only when the conditions it is known to require are met, we would automatically alter our whole approach very radically. We would, of course, make a lot fewer trials and acquire a much smaller daily collection of data. But we would not, I am quite sure, have so many failures brought on by the mistake of calling a badly designed, poorly conducted test a psi failure.

The net result would naturally be to call increased attention to the study of the requirements of the successful subject, the successful experimenter, and the effective experiment as a whole. Much has already been learned about these, but little of it is in shape to be taken seriously and to good effect by a new experimenter. Little of it is, indeed, taken seriously by anyone. The reason for this, I think, goes back to the point of approach that has prevailed. As it has been, the new experimenter has scarcely been able to realize that the task of successfully demonstrating psi could be so unique and so specialized as to be beyond his natural powers and skills. The difficulties have never been sufficiently appreciated and explained. For

that matter, the role of the qualified psi experimenter has never yet been properly presented as one requiring specialized skill. As we all know, in other areas of practice many such specialized arts are already recognized, but the role of the professional psi experimenter is still to be created. If such a role ever is to be successfully developed, we will have to recognize that, just as a test is not to be considered a psi test until it meets certain essential psychological conditions, an experimenter is *not qualified* as a psi experimenter unless he has proved he is equal to the requirements of producing results under conditions that allow for such a demonstration. Obviously, this is saying nothing more than that the common sense which eventually prevails in any profession after a certain stage is reached will apply to experimenting in parapsychology too.

This shift of emphasis, however long it may take, will therefore have to come about eventually. When it does, it will properly place on the experimenter the burden of discovering by actual test whether he is prepared to deal with the elusive phenomena he is setting out to study. It will properly require of the designer of the experiment that the procedure and design be suited to the demonstration of psi before it can, in any proper sense, be considered a psi experiment. This means nothing less than that the characteristics of the subject be taken adequately into account, that the adequacy of motivation and of the preparation of the subject for the test be duly considered, and, finally, that the problems of keeping up the subject's interest and of sustaining good subject-experimenter relations be taken fully into account. It means, in other words, a quite different sort of test approach from that of days gone by.

The whole style of test procedure in psi experiments has been in the past too much dominated by what it was naturally thought in advance that psi must be like. To take an example as we now see it, the emphasis was too much on the search for the supposedly gifted subject, on the assumption that only certain individuals were endowed with psi and that discovering them was the prime objective. Gradually this earlier view, which originally prevailed with us all, began to shift as the facts began to tell against it. Now I think it will have to give way completely to the other extreme: that subjects are *made,* not *born;* that exceptional performance represents a combination of conditions within and around the subject, condi-

tions that favor the functioning of psi to an exceptional degree and may—in fact usually do—continue for only a limited period of time. Thus we must now say only that a good subject is one in whom the right combination of conditions for the demonstration of psi has been found and preserved throughout a series of tests. I have been speaking, of course, only of psi demonstrations and not of psi ability as such; unfortunately, no way has been found as yet to measure even roughly what might be called the native psi endowment in any given subject.

One of the chief advantages which might be claimed for this suggested change of viewpoint is the challenge it gives to each individual participant in the research. No room is left and none should longer be made for the complacent and indifferent attitude of the occasional experimenter in the past who acted as though he might be saying: "Now we shall see if you have ESP. It is no fault of mine if you don't." Rather, it will be a case of the experimenter saying to himself, "My great desire is to inspire this subject to achieve his best."

This attitude will accent the variability of the conditions producing the high scoring and the need to work to preserve those conditions once they are obtained. It reminds us that if we can learn enough more about the facts of psi, we ought to be able to develop subjects who perform better—or at least more lastingly—than have generally been encountered so far. But we shall need to be reminded repeatedly of all this, because even in a field as new as ours and with so few active workers it normally takes years for a new viewpoint to be accepted.

Once such a concept of psi testing does take effect, however, and all of those who take part—the designer, the experimenter, and the subject—come to realize that there is a cooperative effort involved, an art of psi testing can be developed and taught. So long as subjects and even experimenters are ranked as merely "good or bad," however, no experimenter can develop the concept that his job can properly become an art—an art of coaching. Instead, his task is that of a scout setting out to find good subjects as if they were rare but already existent creatures of nature. For those, however, who consider this re-orientation of psi testing worth trying, there is clearly a potential art waiting to be developed. This view-

point should lead to the building of an elementary training program for the professional psi tester, one that should be both selective with regard to suitable personnel and instructive in the testing skills and techniques that are available. Such a course of training should take the blind inefficiency out of the testing program, as selected and effectively trained individuals become prepared and active on the research frontier.

J. B. R.

LETTERS AND NOTES

Precognition, Anxiety, and Aggression

In her recent paper, "Precognition and Intervention" (this *Journal*, March, 1955) Dr. Louisa E. Rhine touches upon a question that in my opinion is of central importance in the many-faceted problem of precognition. Discussing the type of case in which a threatening situation is represented in a dream as ending catastrophically in contrast to the later actuality where, because of the timely and ostensibly dream-provoked intervention of the dreamer, a similar situation does not lead to catastrophe, Dr. Rhine considers the hypothesis of the dual origin of the representations in the dream. She suggests that, given a correctly precognized situation of danger (a child standing unsupervised on the bank of a creek, running toward a cliff, playing near an open window) its catastrophic sequel in the dream could be regarded as "a rational inference derived from the precognitive impression." As to why such an inference happens to take precedence over any of a number of other equally "rational" inferences possible in the state of dreaming to gain particular representation in the dream, Dr. Rhine suggests that: "Inferences such as these presumptive ones might well arise from natural anxieties."

That there are certain anxieties commonly experienced in relation to those to whom we are bound by close ties is true. To consider these anxieties as "natural," however, and to let it go at that, is to pass rather too casually over an area that has been, in effect, systematically ignored in Dr. Rhine's and practically all other serious studies of precognition. To my mind it is the area that more than any other requires exploration and clarification if the problem of precognition is to be advanced beyond our present stage of more or less complete bafflement.

In none of the studies of spontaneous precognition cited by Dr. Rhine, nor in her own, has the relatively high percentage of cases dealing with catastrophic events been considered in itself a datum of possibly great significance. While other types of breakdown of the case material dealt with have been made in different studies, this obvious skewness is taken for granted and either explicitly or by implication accounted for in terms of the "natural anxiety" hypothesis, just as it has been in the collection of spontaneous cases not

involving presumptive precognition. Psychiatric and psychoanalytic investigation is able to demonstrate, however, that when people dream or fantasy about death or accident to parents, spouses, children or friends, the emotions of horror or grief which they experience in the dream or fantasy, or in their recollection, are very likely to be screens for coexisting contrary emotions which are unacceptable to consciousness. It is more or less of a clinical axiom, as a matter of fact—and one that has yet to encounter serious contradiction—that people are not given to dreams or fantasies of death or accident to those to whom they are closely attached, regardless of what feelings of solicitude, sorrow or suffering they may experience attendantly, unless for some reason they harbor a repressed hatred for the very people who are the objects of this concern, a hatred that amounts to an actual wish for their death. Conversely, when these underlying death wishes, as they are termed, are brought to consciousness with appropriate emotional release, the disturbing dreams and fantasies cease.

The psychoanalyst, thus, would be inclined to postulate a repressed destructive wish in every so-called paranormally occurring representation, whether or not precognition appears to be involved, in which a catastrophic event figures. On this basis one could hypothesize that when, in a dream, vision or fantasy, an individual distorts an existing or later reality to include a catastrophic ending which does not in fact occur, this alteration, far from being a "rational inference" automatically given priority because of the individual's "natural anxiety," is expressive of a hidden wish for just such an eventuality. This despite the fact that benevolent feelings may coexist in what is termed an attitude of ambivalence.[1]

The possibility that repressed and ordinarily hidden destructive impulses may play a large part in the phenomenology of spontaneously occurring precognition has been recognized only by Tanagra among the earlier students of the problem. In his monograph *Le Destin et La Chance*[2] and in his paper "The Theory of Psychobolie,"[3] Tanagra carries to the limit (rather bloodthirstily, perhaps) the hypothesis that "a 'premonition' may be a case not of true precognition but of mental force which brings about the imagined event."[4] Other investigators too have considered the possibilities

[1] Eisenbud, Jule. Time and the Oedipus. *Psychoanal. Quart.* In press.
[2] Société des Recherches Psychiques Hellenique, undated.
[3] J. Amer. Soc. psych. Res., Oct. 1949.
[4] *Ibid.*

of "self-fulfillment" in attempting to account for particular cases of presumptive precognition but, so far as I have been able to determine, such a hypothesis has always been quietly withdrawn when it came to explaining catastrophic events occurring to "loved ones." On the face of it this has always appeared to be out of the question. Only Tanagra has had the shockingly bad taste to carry the hypothesis boldly forward to include *all* cases. As a result (certainly not as a consequence of the fact that his theory is any more awkward logically or more crudely expressed than some others conventionally cited) he is never mentioned when the subject of precognition is brought up in the literature.

Sooner or later parapsychology is going to have to take account of some of the ugly facts of life. The latent viciousness and primitive destructiveness within each and every one of us is one of them. Psychiatric and psychoanalytic experience and reflection compel me to align myself squarely alongside Dr. Tanagra when it comes to accepting at least the possibility that *if* it were within the power of the human individual to arrange and bring about the destruction of another—any other, friend or enemy—by an ingenious combination of psi faculties (telepathy, clairvoyance, telepathic influence, PK) such "perfect" crimes would be committed far more often than our rather too prim view of ourselves would allow.

It may be that a thoroughly consistent theory covering the wide range of phenomena subsumed under precognition cannot be constructed solely in terms of Tanagra's major hypothesis. Nevertheless, once we countenance the possibility that human aggression in its manifold aspects, as well as the important problem of man's ingenious defences against his aggression, may well be the nub of the many paradoxes surrounding the problem of "precognition," we are in a position to formulate dovetailing and empirically derived auxiliary hypotheses which, to my mind, may carry us a long way towards a consistent and comprehensive theory. In such a theory, since time as well as space become superfluous parameters, no such difficulty as "crossing the time barrier" is encountered even though the active "self-fulfillment" hypothesis cannot be applied in all cases. On the other hand, the futile attempts to make Time rather than Man the dark villain of the piece are seen in historical, anthropological and psychodynamic perspective as an aspect of the peculiar conditions of man's phylogenetic and ontogenetic development.

This is not the place to go into the many issues that have to be faced and worked through when a thorough reanalysis of our data on "precognition" is attempted from the approach suggested. I might say, however, that as far as I can see, the logical difficulties encountered are comparatively minor. The big hurdles are psychological, such as the need to relinquish once and for all any claim to what is generally (if inaptly) termed "volitional freedom," and the need to face the fact that man, despite his illusions to the contrary, is really no better than nature requires him to be to fulfill her utterly amoral purposes.

But perhaps our first step is to challenge the notion—wherever we see it—that what triggers off the bad dreams that sometimes come true, with or without alterations, are "natural anxieties." Such a notion, and all that goes with it, has long been the gimmick in the biggest scientific muddle of our time.

JULE EISENBUD, M.D.

COMMENTS BY DR. L. E. RHINE

Since my studies of spontaneous psi experiences are frankly exploratory, with only hypothetical implications, I welcome suggestions given on the same hypothetical level. I am therefore glad to have Dr. Eisenbud's suggestion from the Freudian viewpoint as to the possible motivation back of the experiences presented in my recent article. He will doubtless agree, on his part, that the theory he offers has not yet been put to anything like the rigorous experimental test through which the precognition hypothesis, for example, has now passed. In the generalized form in which he uses it, it is rather still on a hypothetical level in spite of its long clinical history.

As is suggested in my article, the subject of intervention, likewise, in spite of its long philosophical history, is still entirely in the hypothetical stage. That stage is, of course, one at which the freest type of exploration is desirable. Accordingly, when the question of motivation is taken up, all the possible ways by which psi (whether of the precognitive or of whatever type) integrates with the driving forces of the personality must be considered. Psychoanalytical hypotheses, along with any others, must be assessed on their merits. Dr. Eisenbud's suggestions will be objectively evaluated according to the dictates of the material itself.

L. E. RHINE.

BOOK REVIEWS

BIRD NAVIGATION. By G. V. T. Matthews. Cambridge, England: Cambridge University Press, 1955. vii + 141 pp. $2.50.

On the factual side, this book serves several useful purposes. It gives an excellent brief survey of earlier investigations of bird navigation, both migratory and homing. It provides a summary of the amazing amount of experimental work on the bird-homing problem by the author during the past six years. The fifteen pages of references will be especially useful to scientists and serious students of the subject, for whom the monograph is primarily intended. The book describes the most significant recent development in experimental method, one to which the author's work has made a major contribution: the observation of the homing orientation of birds at the release point and the statistical investigation of the factors involved. These are only some of the considerations that can be entered on the credit side of the ledger.

But these accomplishments are quite incidental to the author's main purpose, which is to explain bird navigation. He offers a hypothesis based on the sun's apparent movement, essentially as human navigators are able to navigate by the sun with the aid of a sextant, a chronometer, mathematical tables, and charts. For the sake of accuracy, the statement of the hypothesis as applied to birds may be given in the author's own words:

> . . . The essential feature is the *sun-arc*. This is inclined at an angle from the horizontal which is constant for a given place and is a measure of the latitude of that place. Farther north the arc is inclined at a lesser angle, farther south at a greater, so that the highest point on the arc is respectively, lower and higher than at home (Fig. 25). This highest point is due south of the observer in the northern hemisphere and so gives a reference point in space by which the "grid" is related to the surroundings. It is also reached at local noon, and so gives a reference point in time. The speed at which the sun moves round its arc is, for practical purposes, constant, at 15° an hour. When it has reached a particular point on its arc at home, it will have advanced further to an observer in the east, and less far to one in the west, the differences in arc angle (the angle round the arc from the noon position) being directly proportional to the change in longitude.
>
> At home the bird will become familiar with the features of the sun-arc, and the sun's position on it at different (local) times. These will

be related to the internal "chronometer" which is also an essential part of the hypothesis. In unfamiliar surroundings the bird will have to construct the sun-arc from observation. The suggestion is that it observes the sun's movement over a small part of its arc and extrapolates to obtain the highest point. Measurement of the altitude of this point, the angle from the horizontal, and comparison with the remembered value for home, say, the previous day, will give the latitude change. The arc angle from the observed sun to this highest point when compared with that obtaining at home for the same chronometer time will give the longitude change (pp. 92-94).

What is the "direct" evidence offered in support of this hypothesis? It consists of two "waiting" experiments which took their name from the fact that the birds were shut in at home for several days near the time of the September equinox without being able to see the sun, sky, or shadows. This time of the year was selected because it is the period when the noon elevation of the sun is decreasing most rapidly. At the end of the period of confinement the birds were taken under cover to a southern point, but not far enough to compensate for the drop in the sun's elevation. It was predicted that the birds, finding the sun *lower* in the sky at the release point than when they last saw it at home, would react as they would if they had been taken north without the waiting period. Eighteen experimental birds started off in a general direction away from home as predicted, and four flew toward home. Control birds imprisoned at home for the same period but with a view of the sun were released separately at the same points on the same two occasions, and they started off preponderantly within the home half of the circle.

This is an interesting result, but not enough in itself to prove the sun-navigation hypothesis. A reasonable scientific estimate of the value of these results would be that they called for repetition of the experiment by the same investigator with different birds and release points and confirmation by other workers before they could be taken as proof of sun navigation. But not only did Dr. Matthews not do more work along these lines before reaching a conclusion; he also discounts waiting experiments by other investigators which failed to confirm his results (p. 107). Two such experiments were carried out in Germany at the Max-Planck-Institut, Wilhelmshaven, one of them being a two-experimenter investigation (Mr. and

Mrs. K. S. Rawson). In both of these experiments, homeward departures were observed in birds confined *without* a view of the sun as well as by those *with* it.

Dr. Gustav Kramer, the third of these experimenters, has questoined the logic of the "waiting" experiment in his report, which is cited in the book without any mention of the criticism. Briefly, Dr. Kramer's point is that the change in midday elevation of the sun as brought about for the birds in the waiting experiment would lead to the behavior predicted by Dr. Matthews on his hypothesis only if the birds were released at the southern point near noon. At this time the seasonal change might conceivably be perceived by the bird as a decrease in noon elevation of the sun and lead to orientation away from home. But during the waiting period the sun path has undergone changes characteristic of other times of day as well. For example, if the birds denied a view of the sun for several days were taken south and released in the *forenoon*, the most conspicuous change in the sun would be the lower position on its path, which by the Matthews hypothesis should result in flights toward the west. Dr. Matthews predicted northward flights regardless of the time of day. He assumed, in other words, that birds released in the forenoon would react to the slight change in the *slope* of the sun's path instead of to the relatively great change in its *position*. Although this criticism does not rule out the possibility that the waiting experiment could in the future produce evidence in support of the Matthews hypothesis, it does indicate that such evidence as has been produced by it thus far weighs *against* sun navigation rather than in favor of it.

Equally inconclusive are other lines of evidence offered as supporting the sun-navigation hypothesis. One basic assumption of the hypothesis is that the bird possesses a chronometer which is accurate within a limit of a very few minutes over a period of several days (the maximum time that can elapse after the bird is displaced from home without the loss of orientation ability). Furthermore, once this "clock" has been set at home, it must continue to function independently of changes in day-night intervals encountered on the outward journey, for it must be used to check upon the sun's movement. Of course, the existence of a general appreciation of time in birds had already been demonstrated in earlier experiments.

The investigations of Dr. Kramer and his associates are of particular interest in this connection because they showed that the birds also take account of the shift in the general position of the sun throughout the day. It is a big step to go from such a general appreciation of time, however, to the precise internal chronometer required by the sun-navigation hypothesis. If such precise estimates of time are made by birds, it should be possible to prove the fact experimentally. This objective could well be an early part of any sound scientific program of research to test the sun-navigation hypothesis of homing.

Another basic requirement of the Matthews hypothesis which remains in the realm of mere assumption is the ability for measuring the curvature of the sun's motion along a small fragment of its pathway. To enable the bird to achieve this while in flight, it is assumed to have a built-in horizon mounted in its head that "moves forward with rocklike constancy" (p. 109) during flight. But precise determination of the curve is not all that is required. The bird must make an accurate extrapolation of the sun along this curve to the zenith and must appreciate both the *time* of reaching the highest point and the difference in elevation between the *inferred* point where it is released and the *remembered* highest point of the sun at the loft. Much emphasis is given in the book to the remarkable avian eye which, in addition to a structure of unexplained function found on the retina (the pecten), is said to have a theoretical limen for the perception of angular motion "between 250 and 500 times greater than that required for orientation at 50 miles" (p. 108). But is not this comparison irrelevant to the basic requirements of the sun-navigation hypothesis? What does the bird's ability to perceive *motion* under theoretically optimum conditions have to do with making a comparison between how the sun looked at the home location yesterday and how it looks today, at a spot 50 miles away?

The fact that pigeons show little evidence of homeward orientation in strange territory if the sun is hidden by clouds is offered as an argument for the hypothesis. But this fact, upon which most investigators of homing agree, is interpretable in other ways. The author mentions two explanations that have been suggested, one

by the present reviewer which he attempted to test experimentally and the other by Dr. Kramer against which he argues.

The former suggestion was that birds may not like to fly when the sun is not visible, and that this psychological disturbance may be sufficient to inhibit the orientation process. Dr. Matthews released birds repeatedly at the same points and showed that after six flights they showed homeward orientation with a complete overcast. Presumably, however, the birds had learned landmarks for the complete homeward journey, and they no longer had to rely on their orientation ability which ordinarily comes into play in *strange* territory. So this series of experiments really does not bear upon the original suggestion that the absence of the sun may inhibit that orientation response.

Dr. Kramer's suggestion regarding the disturbing effect of overcast skies is that the birds have a yet-to-be-discovered means of appreciating their position relative to the loft, but that they may rely upon the sun as a reference point for keeping on the course after the initial response of homing orientation has been made. The book offers nothing in the way of experimental evidence against this view.

What the book has to say about the possibility of an ESP factor in homing is contained in Chapter 6, "Maintenance of Sensory Contact with Home," as part of a paragraph which opens with the topic sentence: "Odd theories calling for 'radiations' of an unspecified nature from the home area frequently crop up in popular literature" (p. 72). The reference to parapsychology is so brief that it may, because of its special interest to readers of the *Journal*, be quoted in full:

> The parapsychologists Rhine (1951) and Pratt (1953) have suggested that some extra-sensory means of orientation is the basis of homing. However, even if we could accept their findings as to the reality of telepathy, clairvoyance and psychokinesis in Man (and Spencer-Brown [1953] has made a sharp attack on the logical and statistical nature of their evidence), it is difficult to see how such phenomena, if they existed in animals, could be of assistance in homing. Their independence of ordinary considerations of time and space has been strongly emphasized by their advocates, so they could hardly be of use for two-dimensional orientation. Indeed, no suggestions as to the mode of operation have been forthcoming from the parapsychologists, who really became interested in bird navigation only because the known facts had then

received no adequate explanation in terms of sensory physiology (pp. 72-73).

There are a few things about this statement that require comment.

What may seem like a small point for disagreement is the use of the verb *is* in the first sentence instead of some conditional form like *may be*. However, anyone who has read the two articles referred to (as presumably Dr. Matthews has done) will recognize that they only raised the question whether ESP may be a factor in animal behavior, and did not suggest that it *is* so. Whether intentionally or not, the wording chosen by Dr. Matthews implies that something was claimed beyond this mere question-raising stage.

The implication that the views of Mr. Spencer-Brown justify some reservation about the reality of ESP in humans reflects a lack of knowledge of the evidence. Without a doubt, the strength of the case for ESP in humans is far greater than that for directed bird homing in a novel direction in strange territory. This is not to imply that the evidence for the latter is not sufficient to establish it, but is only to point out the apparent inconsistency in standards of evidence. Perhaps it should be said in passing that if Mr. Spencer-Brown's views have any cogency for statistics, they would apply to pigeon homing data as much as to ESP results.

But even though Dr. Matthews begrudgingly assumes ESP in humans, he implies that the question of an ESP factor in homing should not be raised unless there is first available an explanation of how such a factor would operate. His objection that the degree of independence from space-time limitations found for ESP would make it inapplicable to the homing situation shows a misunderstanding of the evidence. It is true that some human subjects have succeeded as well in ESP tests conducted over many miles as in tests from one room to another. This fact does not mean, however, that the response is occurring completely outside any space-time framework. In fact, the degree of orientation ability required in an ESP subject to respond to the order of cards in a concealed pack in a laboratory 100 miles away is, if anything, far greater than that required in a bird in turning toward a familiar home territory at the same distance.

Why should we offer an explanation of how ESP could serve

as a basis for homing before we have answered the question whether ESP is involved in this behavior? The history of research on bird navigation shows that the homing problem has been repeatedly complicated by explanations offered on purely speculative grounds. What is needed at this stage is a better foundation of soundly established facts upon which working hypotheses, tentatively formed as guiding viewpoints for the research, may be based. Let us first find out whether ESP plays a part in homing and if it does, then raise questions regarding the nature of this function. The scientific propriety of this order of events seems self-evident.

Dr. Matthews' final remark in the above quotation seems to mean that the parapsychologists, who waited so long before taking an active interest in bird navigation, could have saved themselves a lot of trouble if they had waited just a few years more, for the sensory basis has now been found in sun navigation, and the parapsychologists really have no reason to concern themselves any longer with the problem!

The author insists that the absence of any acceptable alternative sensory explanation is a strong argument in support of sun navigation. Even if we grant that no other sensory hypothesis is in a strong position at the present time, we may disagree with the favorable inference for sun navigation drawn from this fact. The present knowledge about bird navigation leaves the way open for an explanation either within the realm of sensory physiology or within the area of parapsychology. Claiming special privileges for a personally-preferred hypothesis will not alter the scientific status of the research. This reviewer even willingly concedes that, before the solution is reached, further serious consideration may have to be given to the role of the sun in this behavior. This estimate, however, does not change his opinion that *Bird Navigation* draws conclusions favorable to the sun-navigation hypothesis which are wholly unwarranted at the present time.

J. G. Pratt

La Parapsychologie. By Robert Amadou (with a preface by D. J. van Lennep). Paris: Denoël, 1954. Pp. 369.

This is the best survey of the whole field of parapsychology that has yet appeared in French. M. Amadou gives more of the historical background than is usual in a comprehensive work, but

at the same time he is up to date in his information about recent developments. His views on the more controversial issues reveal a cautious, critical attitude. For instance, on the matter of precognition, he draws attention to the few quantitative experiments that furnish reasonable evidence (p. 277). He also repeatedly points out that the assumption that ESP is independent of space and time is by no means established experimentally. As to psychokinesis, he reserves judgment. He criticizes the conditions of the earlier experiments at Duke and points out that, apart from position effects, positive PK results under good conditions are mostly so small and marginal as to lend plausibility to Spencer Brown's hypothesis of a statistical peculiarity (p. 280). Physical effects produced by mediums he does not deal with (as he has written a separate book on this topic), but he states that, on the available evidence, one can neither affirm nor deny the existence of such phenomena (p. 71).

M. Amadou regards the attitude of the spiritualists, who attribute parapsychological phenomena to the activity of departed souls, as a religious viewpoint and quite outside the scientific domain (p. 305). In his opinion the spiritualist approach simply glosses over the need for scientific explanations, just as the once fashionable religious belief in a fixed moment of creation avoided the scientific problems of evolution. The only form of survival hypothesis that, in his view, approaches scientific intelligibility is that of the temporary persistence of mental elements capable of inspiring a medium (p. 321). He rejects the orthodox Christian interpretation, according to which paranormal phenomena are miraculous interventions, sometimes divine and sometimes diabolical, as being as much outside science as the spiritualist approach.

On one point M. Amadou is particularly and fiercely critical. He deplores J. B. Rhine's assertions that parapsychological findings can be used to combat Marxism and materialism. He finds no justification for associating the findings with ideological controversy, and he thinks that this sort of attitude confuses the uninformed, who cannot readily distinguish between unwarrantable deductions and reasonably established facts. In this connection he quotes Pasteur:

"Il n'y a ici ni religion, ni philosophie, ni athéisme, ni maté-

rialisme, ni spiritualisme qui tiennent. C'est une question de faits."

M. Amadou is strong on historical references. It is interesting to read, for example, that the term parapsychology was proposed by Max Dessoir in 1889 (p. 13), and that, as far back as the seventeenth century, Athanase Kircher, a famous Jesuit, explained that the movements of divining pendulums were due to the operators' unconscious muscular activity. These and many other allusions are documented by a valuable collection of bibliographical notes and references. (Unfortunately the book as a whole lacks a subject index.) M. Amadou has certainly combed the literature, and in so doing he has given equal prominence to British, American, and Continental sources, thereby adding considerably to the interest of his book. To give but three examples out of many, his description of Dr. Allenby's psychoanalytical investigation of ostensibly precognitive dreams (p. 265), his reference to Franke and Koopman's discovery of peculiarities in the electroencephalographic records of entranced subjects (p. 196), and the brief mention of the exploits of a contemporary clairvoyant medium, Mme. Marie (p. 141), cannot fail to arouse the interest of English-speaking readers who are mostly unfamiliar with these researches.

Unfortunately, while casting far and wide for interesting data, M. Amadou did not always make a wise selection of material. His presentation of the experimental evidence in particular is open to the criticism that he gives undue prominence to dubious work. For instance, he devotes over three pages to a favorable description of Hettinger's work, but with no account of the criticisms which completely vitiate Hettinger's results. In contrast, his account of the classic Pratt-Woodruff experiments is limited to just over a page, and the Pearce-Pratt series is given only a few lines (p. 172). He also cites Dr. Urban's statistically impressive ESP results with subjects after electroshock treatment, but he fails to indicate the highly unsatisfactory conditions in which these tests were performed, and he also fails to mention Dr. Betty Humphrey's inability to confirm Urban's results (p. 201). A serious inaccuracy is the statement that Denys Parsons' results with his counter machine for testing pure clairvoyance were "clearly significant" when in fact they were null (p. 213). (See *Proc. S.P.R.*, Vol. 48, 1946, pp. 28-31.)

In discussing the relative merits of the qualitative and the statistical approach—which he does at length—the author comes down on the side of statistically controlled experiments as the chief source of reliable scientific evidence. Rather illogically, he also regards the older, uncontrolled tests as somehow more informative, since they study ESP in its natural surroundings. In fact he bases most of his theorizing on observations of a so-called qualitative type. For example, he concludes that hypnotic and trance states favor the manifestation of ESP. M. Amadou is so up to date in other respects it is surprising to find him adhering to this viewpoint. The reason why no great reliance nowadays is placed upon these early findings is not that they were of a qualitative or non-statistical nature—since the results were often far too obvious to require statistical evaluation—but that, considered by present-day standards, the experimental conditions were usually very poor. If only we could today obtain the same striking results from hypnotic and mediumistic subjects working under good conditions of control, then there would be more justification for M. Amadou's standpoint.

D. J. West

Physical and Psychical Research. By C. C. L. Gregory and Anita Kohsen. Reigate, Surrey: The Omega Press, 1954. ix+213 pp. 15s.

The objective of science most generally subscribed to is the augmentation of control over the conditions of man's existence. For the last several hundred years it began to look as if there were almost a linear relationship between the amount of knowledge secured by science and the degree of control achieved. But something in this rosy picture has changed. We are now becoming aware that the fast increasing complexity of man's scientific behavior has itself introduced a factor which threatens to act in the direction of disorganization and decontrol. We have developed and exploited vast resources for getting information only to find that we have no adequate means of providing for general understanding of what we have got (or even understanding among those whose business it is to understand these things), of distinguishing between

true information and false information, of gearing our information to what one would imagine to be the primary factors of human resources and human needs, or even of codifying and storing information. Man's anxiety, which might conceivably be a gauge of the amount of randomness in the world he inhabits, seems steadily to be mounting.

In *Physical and Psychical Research,* the authors, described in the jacket blurb as a physicist with training in psychology, and a psychologist especially interested in lower animal behavior, have set out to survey the present mess in science and to make suggestions for a more fruitful use of our methodologies and our data.

In Chapter I, "General Considerations," the authors state that "four functions are commonly employed in deriving the sort of knowledge with which we are best satisfied": logico-deductive synthesis, inductive synthesis, the operational method, and intuition. These four methods are exemplified with problems from physics, biology, and animal ethology. It is shown that very often in the description of certain facts one method of description can adequately replace another, and this is illustrated by the substitution of the concepts of information theory, made possible by the science of cybernetics, for the classical description of certain events in terms of energy transfer. After demonstrating in connection with concrete problems the advantages and disadvantages of each of the four methods of arriving at knowledge taken separately, the authors state that "all four belief criteria must be complied with by any systematization of knowledge capable of providing a sure foundation for a unified science."

In Chapter II, "Belief or Methodology," the authors attempt to show that any one of the four methodologies is too restrictive if used as an exclusive criterion of belief. "We would now put the question as to whether it is at all possible that a generally acceptable methodology, based on our four belief criteria, may be found to replace belief systems in regard to the function they perform in preserving science from chaos and superstition. We hope that this can and will be accomplished, providing a new systematics can be evolved, which starts from describable experiences fully supported by our four criteria. Such a methodology is envisaged as providing a framework for communal experience which will not hamper and

restrict either the less fully determinate phenomena or the necessary hypotheses for their subsumption." In general, the authors advocate the loosening up of the logico-deductive in order to admit of data (e.g., those of parapsychology) which do not at first appear to be in accord with a prevailing world-picture, and the tightening up of the operational, in order to insure that data will not be violently manhandled.

In Chapters III and IV the authors expand their thesis in terms of detailed discussions of relativity and psychoanalysis respectively. In the first they show that neither Einstein's nor Milne's relativity theories, both comprehensive conceptual systems relying heavily on the logico-deductive approach, provide an adequate account of all the physical data which have been established. In the chapter on psychoanalysis the authors provide a lively description and summary of what goes on in this field, both theoretically and empirically, and then point out the paradoxes and absurdities which result from the inadequate conceptual system in use. They attempt a more satisfying description of some of the major psychopathological pictures in terms of communication theory as it might be applied here by an investigator whose basic orientation came from the animal ethological approach. Both physics and psychoanalysis, they suggest, would do well to lean more heavily toward the operational.

Coming to "Psychical Research," Chapter V (and the last), the authors assert that attempts to incorporate paranormal events into our belief system have not met with much success since our official belief system is to a great extent circularly self-validating. ". . . all world views, which could at present be regarded as 'scientific,' are partial and incomplete, not only because they have not, so far, described all possible objective occurrences, but because their very language is incapable of describing such events." With a bow to Professor Price, the authors then try to see how far they can go using the notion of "images" as a central concept in a framework of communication theory, again as elaborated by someone with the operational background of an animal ethologist. "If information be conceived of as primary, it must be asked how we can obtain information about images?"

The trouble is, hold the authors, that we usually don't think of images as "objective" things. "The idea that images may be at

least the precursors, and possibly the 'causes' of physical happenings, is quite foreign to our usual mode of thought; but it is just this idea which is central to our present inquiry." They then proceed to develop the properties of images, complex image centers and their hierarchies, arriving at the proposal that "haunting" be regarded as "the central concept of parapsychology." "This (haunting) we conceive to be a relational property between centers, and between centers and objects, in addition to any other informational relationship, such as sensory or nervous links. Thus it will be seen that haunting is merely another name for our imagined information loops, which we invoked for describing hierarchical binding within an individual, both animal and human." Advantages from the use of such a concept, according to the authors, accrue not only to psychical research but also to general psychology, where it could be applied to such phenomena as identification, hypnotism, and healing, among others. Ostensibly as a consequence of their concept of haunting and hierarchies, the authors suggest, apropos of research, that "It is not simply a question of providing incentives, but rather of providing a setting where such behavior patterns would be appropriate." Further: "There is at least some evidence that with groups constituted for the purpose of carrying out magical acts such as those once described as necromancy, queer happenings do occur. If the paranormal be largely the outcome of specially motivated group activities, perhaps we should consider rather the desirability of developing such arts than of establishing a latent tendency in a few unusual people to accomplish them."

The final plea of the authors is given in the last paragraph of the book: "We suggest this is the time to develop 'metascience,' which might perhaps be called nexology, its subject-matter being the informational relations between centers, individuals, and groups. Freed from a too narrow conceptualism and the ideological, it would concentrate on methodology, the operational, and communication between all that is informationally related."

The foregoing is a bare and all-too-inadequate account of the drift of the gist of the content of this unusually provocative book. This reviewer found it also an exasperating book, a sad mixture of lucidity and not just obscurity but downright obscurantism, frequently hard to follow because of its lack of proper organization and

uneven exposition. Terse, cramped statements, written as if the authors had to say all they wanted to say within the confines of a short paper, are followed by meandering and sometimes discursively irrelevant pages, as if they were embarking on a sixteen volume work and had decided that they might as well put in just about every tangential consideration, anecdote, and suggestive conjecture they could think up. Definitions of terms are often sloppy and inadequate for helping the reader to find out just what the authors are talking about. (Compare in this respect several recent volumes on cybernetics and communication theory which contain detailed glossaries precisely *because* this new field has not yet steadied out into generally acceptable definitions of some of its primary terms and concepts.) The situation, furthermore, is not helped by the absence of an index, making it difficult for the reader to refer back to threads he may have lost en route. All in all the book seemed regrettably lacking in just those "loops" ("informational" or plain horse-and-buggy rhetorical) which might better have connected the authors, ardent though they may be in the abstract as proponents of communication theory, and their readers. The reviewer found himself wishing that the spirit of William James, to whose memory the book is dedicated, had hovered over the pages of this book in a somewhat more effective demonstration of beneficent haunting.

Despite these objections, which might well be considered fatal in the case of almost any other book, *Physical and Psychical Research* is on the whole worth the struggle. It presents in a challenging way an approach to many issues, in and out of parapsychology proper, which are much in need of some sort of clarification. Whether the type of meta-language for a meta-science proposed by the authors represents the worthwhile beginnings of a true clarification or merely a pseudo-clarification which will land us back not far from where we started when the smoke lifts (and of smoke there is plenty in this "revolution," as the authors term what they are attempting) is difficult to say. There seems little doubt of the fruitfulness of information theory when applied to problems of computing machines and their analogues; but as a model for the problems of human psychology, even when basted up with the trimmings of operationalism and animal ethology, it looks a little thin, and I, for one, am hardly impressed with the authors' attempts to describe psycho-

pathological behavior in such terms. Quite apart from the fact that I cannot quite see what is to be gained heuristically from some of the fancy retranslations (e.g., barring the uncouth and thoroughly abhorred "unconscious" from the front door and then bringing it around through the rear as "pretenses," "innate images," etc.) I am completely at a loss to figure out what to do with this new language in the face of even one concrete clinical problem; and I can think of no problem I have even seen that does not have important facets to it that do not appear for the most part incapable of being described in the language proposed. Until the authors (or their sources) show me how to meet this situation in the only operational terms that mean much in the overcrowded clinic, I shall feel obliged to fall back regressively upon the slatternly ways of present-day psychoanalysis.

The issue of "What can it do, not what can it say?" arises also in connection with the application of information cum ethological theory to parapsychology. Here one cannot help but be fascinated by the vigor of some of the authors' concepts (when not obscured by turbid presentations) and by the dexterity of their manipulations; but at the end one realizes that they have provided no hypotheses for testing which are specifically and exclusively entailed in their own way of formulating things. The suggestions advanced for research do not result any more from the language they have chosen to speak than from the way ideas and insights seem to be shaping up anyway in the empirical mosaic of parapsychology. How then is it possible, in terms of communication theory, to judge the proposed language system in respect to its capacity to convey information as distinguished from its capacity to yield only noise?

Nevertheless attempts such as the authors of this book have made to challenge some of the assumptions of our current conceptual systems are to be welcomed. I suspect that some of the ideas presented will turn out to be more serviceable when more systematically developed and tested.

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GLOSSARY

In order to avoid constant redefining of commonly recurring terms in papers appearing in this JOURNAL, the following definitions are submitted for convenient reference.

A simple description of the basic experimental methods and of the statistical procedures to be followed in evaluating ESP (both card tests and drawings tests) and PK may be found in the *Handbook of Tests in Parapsychology* by Betty M. Humphrey (published by the Parapsychology Laboratory, Duke University, Durham, N. C.).

AGENT: In tests for telepathy, the person whose mental states are to be apprehended by the percipient. In GESP tests, the person who looks at the stimulus object.

AVERAGE SCORE: Average number of hits per run.

BT ("Broken Technique"): The clairvoyance technique in which each card is laid aside by the experimenter as it is called by the subject. The check-up is made at the end of the run.

CALL: The ESP symbol selected by the subject in trying to identify a target.

CHANCE: The complex of undefined causal factors irrelevant to the purpose at hand.

CHANCE EXPECTATION = MEAN CHANCE EXPECTATION: The most likely score if only chance obtains.

CHANCE AVERAGE: Mean chance expectation expressed as an expected score, generally in terms of average per run.

CHI SQUARE: A sum of quantities each of which is a deviation squared divided by an expected value. Also a sum of the squares of CR's.

CLAIRVOYANCE: Extrasensory perception of objective events as distinguished from telepathic perception of the mental state of another person.

CR (CRITICAL RATIO): A measure to determine whether or not the observed deviation is significantly greater than the expected random fluctuation about the average. The CR is obtained by dividing the observed deviation by the standard deviation. (The probability of a given CR may be obtained by consulting tables of the probability integral, such as Pearson's.)

CR OF THE DIFFERENCE: The observed difference between the average scores of two samples of data divided by the standard deviation of the difference.

DECK: Twenty-five ESP cards.

CLOSED DECK: An ESP deck composed of five each of the five symbols.

OPEN DECK: An ESP deck made up of the ESP symbols selected in random order.

DEVIATION: The amount an observed number of hits or an average score varies from the mean chance expectation or chance average. A deviation may be total (for a series of runs) or average (per run).

DIE THROW: The elementary unit of data in a dice-throwing test represented by the throwing and reading of a single die, no matter how many dice are thrown at a time.

DISPLACEMENT: ESP responses to targets other than those for which the calls were intended.

BACKWARD DISPLACEMENT: ESP responses to targets preceding the assigned targets (the ones for which they were intended). Displacement to the targets

one, two, three, etc. places preceding the assigned target are designated as (−1), (−2), (−3), etc.

FORWARD DISPLACEMENT: ESP responses to targets coming later than the assigned targets. Displacement to the targets one, two, three, etc. places after the assigned target are designated as (+1), (+2), (+3), etc.

DT (Down Through): The clairvoyance technique in which the cards are called down through the deck before any are removed or checked.

EMPIRICAL CONTROL: An experiment which wholly or partially follows the main experiment with the exception that the conditions are designed to exclude the possibilty of the hypothesis being tested.

ESP (EXTRASENSORY PERCEPTION): Response to an external event not presented to any known sense.

ESP CARDS: Cards, each bearing one of the following five symbols: star, circle, three parallel wavy lines (called "waves"), square, plus.

ESP SYMBOLS: See plate opposite page 1, this JOURNAL, Vol. 1, March, 1937.

EXPECTATION; see CHANCE.

EXTRACHANCE: Not due to chance alone.

FREE MATERIAL: The stimulus objects of experiments in which an unlimited or unspecified range of stimulus objects is employed (as contrasted with methods such as card-calling in which the subject knows that the stimulus object is one of a known range).

FREE RESPONSE METHOD: An ESP test method in which a relatively unlimited range of stimulus objects is used and in which the subject is instructed to express freely as his response any range of thoughts and ideas.

GESP (GENERAL EXTRASENSORY PERCEPTION): A technique designed to test the occurrence of extrasensory perception, permitting either telepathy or clairvoyance or both to operate.

HIGH-DICE TEST: A PK technique in which the aim of the subject is to try to influence a pair of dice to fall with the two upper faces totaling eight or more.

LOW-DICE TEST: A PK technique in which the aim of the subject is to try to influence a pair of dice to fall with the two upper faces totaling six or less.

MEAN CHANCE EXPECTATION; see CHANCE.

P (PROBABILITY): A mathematical estimate of the expected relative frequency of a given event if chance alone were operative.

PARAPSYCHOLOGY: A division of psychology dealing with those psychical effects which appear not to fall within the scope of what is at present recognized law.

PERCIPIENT: The person who makes the calls in an ESP test.

PK (PSYCHOKINESIS): The direct influence exerted on a physical system by a subject without any known intermediate physical energy or instrumentation.

PRECOGNITION: Cognition of a future event which could not be known through rational inference.

PREFERENTIAL MATCHING: A method of scoring free responses. A judge ranks the stimulus objects (usually in sets of four) with respect to their similarity to, or association with, each response; and/or he ranks the responses with respect to their similarity to, or association with, each stimulus object.

PSI: A general term to identify personal factors or processes in nature which transcend accepted laws. It approximates the popular use of the word "psychic" and the technical one, "parapsychical."

PSI PHENOMENA: Occurrences which result from the operation of psi. They include the phenomena of both ESP (including precognition) and PK.

QD (QUARTER DISTRIBUTION): The distribution of hits in the record page (or in a logical subdivision thereof, such as the set or the half-set) as found in the four equal quarters formed by dividing the selected unit horizontally and vertically.

RUN: A group of trials, usually the successive calling of a deck of 25 ESP cards or symbols. In PK tests, 24 single die throws regardless of the number of dice thrown at the same time.

SCORE: The number of hits made in one run.

TOTAL SCORE: Total of scores made in a given number of runs.

AVERAGE SCORE: Total score divided by number of runs.

SD (STANDARD DEVIATION): Usually the theoretical root mean square of the deviations. It is obtained from the formula $\sqrt{npq}$, in which n is the number of single trials, p the probability of success per trial, and q the probability of failure. (For ESP cards, SD $= 2\sqrt{\text{no. of runs.}}$) Sometimes the SD used is that estimated from the observed variability in the scores.

SD OF THE DIFFERENCE: For both ESP cards and PK tests using dice, the SD of the difference is equal to $\sigma_s \sqrt{1/R_1 + 1/R_2}$ where σ^2 is the SD of a single run and R_1 and R_2 are the number of the runs in the respective samples compared. This gives the SD of the difference for run score averages.

SERIES: Several runs or experimental sessions that are grouped in accordance with a stated principle.

SESSION: A unit of an ESP or PK experiment comprising all the trials of one test occasion.

SET: A subdivision of the record page serving as a scoring unit for a consecutive group of trials, usually for the same target.

SEVENS TEST: A PK technique in which the aim of the subject is to try to influence a pair of dice to fall with the two upper faces totaling seven.

SIGNIFICANCE: A numerical result is significant when it equals or surpasses some criterion of degree of chance improbability. The criterion commonly used in this JOURNAL is a probability value of .02 or less, or a deviation in either direction such that the CR is 2.33 or greater.

SINGLES TEST: A PK technique in which the aim of the subject is to try to influence dice to fall with a specified face up.

STIMULUS OBJECT: The ESP card or drawing or other object, some identifying characteristic of which is to be apprehended by the percipient.

SUBJECT: The person who is experimented upon. In ESP tests, most commonly the percipient (though also the agent in GESP and telepathy). In PK tests, any individual whose task it is to influence the objects thrown.

TARGET: In ESP tests, the stimulus object; or in telepathy, the mental state of the agent. In PK tests, the faces of the die (or combination of faces) which the subject attempts to bring up in the act of throwing.

TARGET CARD: The card which the percipient is attempting to perceive (i.e., to identify or otherwise indicate a knowledge of).

TARGET DECK: The deck of cards the order of which the subject is attempting to identify.

TARGET FACE: The face on the die which the subject tries to turn up as a consequence of direct mental action.

TELEPATHY: Extrasensory perception of the mental activities of another person. It does not include the clairvoyant perception of objective events.

TRIAL: In ESP tests, a single attempt to identify a stimulus object. In PK tests a single throw of the dice or other objects thrown.